Joint Review of[1]

A Mathematician's Apology
by G.H. Hardy
Publisher: Cambridge University Press
$12.00 paperback, new. Much cheaper used. Also free online.
154 pages. Original Year: 1940
Forward by C.P. Snow added for 1967 edition
Several editions. The latest was from 2012

and

An Applied Mathematician's Apology
by Lloyd N. Trefethen
Publisher: Society for Industrial and Applied Mathematics
$36.00 hardcover, 80 pages, Year: 2022

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1 Introduction

In 1940 G.H. Hardy wrote *A Mathematician's Apology* which argued why Pure Mathematics is a worthy endeavor. The book makes some other points and also seems to dismiss Applied Mathematics as dull. The book was around 80 pages, though with the later editions long introduction by C.P. Snow, it is 154 pages.

In 2022 Nick Trefethen (he goes by his middle name Nick, not his first name Lloyd) wrote a book that looks at the tension between pure and applied mathematics, defends applied math against Hardy's dismissal, and also makes some criticisms of some types of applied math. This book is also around 80 pages, so the same length as Hardy's book.

We review both books here and give an opinion of who is right. Spoiler Alert: Trefethen.

2 A Mathematician's Apology

Hardy begins by saying that works *about Math* (like the book he is writing, and that is why he is bringing this up) are somehow inferior to books that *are Math*. In what sense? He doesn't really say, which is why I find this point of view misguided[2]. He seems to also think that people should do what they are good at, and that most people are not good at anything. An intelligent discussion about what people are good at, nature vs. nurture, what jobs require what talents, etc., would have been interesting. But none of that is here.

Hardy says that Mathematics is a young man's game and claims that no great discoveries in Mathematics have been made by anyone over 50. He gives as examples (1) Gauss, Newton, Painleve (who?), and Laplace which are fair examples, and (2) Galois, Abel, Ramanujan, Riemann, who all died before they were 50, so these are unfair examples. In my mind Hardy has made an *interesting conjecture* rather than an established fact.

[2] I may be biased since I've written over 100 book reviews and over 20 open problem columns, and write a blog, most of which are *about Math*.

Since Hardy's reasoning is speculative and not rigorous, I will do the same. The following people made important contributions after 50: Rabin , Valiant, Wigderson, Apery ($\zeta(3) \notin Q$ at the age of 62), Yitang Zhang (bounded gaps between primes at the age of 58), Louis de Branges (proof of Bieberbach's Conjectures at age 51), Andre Weil and Jean-Pierre Serre did high-level work past 60. In Serre's case past 80. Michael Rabin , Les Valiant, and Avi Wigderson continued to do great work past 50. Valiant and Wigderson are still active.

This may be unfair to Hardy since the people above were mostly not born before Hardy died. It would be an interesting history project to see which mathematicians before Hardy produced great work after 50. Archimedes may be an example, but our records from that time period are terrible. Euler is a better candidate. Also, Weierstrass proved his approximation theorem at the age of 70.

I believe that people over 50 are *capable* of doing great mathematics; however, there are reasons why it is rare:

1. (Andrew Gleason told me this one.) A mathematician works in one field for a long time and the field dries up: all of the open problems left are too hard to solve at that time. Since mathematics is an old field with much knowledge already built up, changing fields is hard. So the mathematician is stuck. This is why Computer Science, a newer field, has not had this problem as much.

2. The Peter Principle: Absola is such a great researcher, let's make her department chair so she will have no time for research.

3. In Hardy's time people often died before 50.

And now for the two main courses: (1) Why does Hardy think pure math is a worthy endeavor? and (2) What did he think of applied math?

Pure Math. Hardy says that while some math is useful (more on that later), that is not the real reason why math is worth doing. He gives as examples of beautiful mathematics (a) the proof that $\sqrt{2}$ is irrational, and (b) the proof that the primes are infinite.

He talks about why these theorems, and others, are beautiful and deeper than examples with small numbers (e.g., the Theorem that 5 is the sum of 2 squares) or chess. He talks about significance, depth, and generality. He admits that these terms are hard to define. Okay, so in the end was all of this interesting? Convincing? *Interesting?* Yes, its good to see someone struggle, as we all do, to defend our field. *Convincing?* No. I already believed that pure math is worth doing for similar reasons that Hardy did, though he articulates them well. Even so, I doubt this would convince someone who had not already drunk the Kool-Aid.

Applied Math. I can do no better than give a quote which reflects what Hardy says in several parts of the book (I leave out some boring side comments):

> It is undeniable that a fair working knowledge of the differential and integral calculus has considerable practical utility. These parts of mathematics are, on the whole, rather dull. They are just the parts that have the least aesthetic value. The 'real' mathematics of the 'real' mathematicians, like Fermat and Euler and Gauss and Abel and Riemann, is almost wholly useless (and this is as true of 'applied' math as of 'pure' math).

He claims that Relativity and Quantum Mechanics use interesting math (I would say they also motivated interesting math) but are not practical. He was right about that in his time, but wrong in the long term. And of course he thought Number Theory was interesting math that was not practical. He was wrong about that as it is now used heavily in Cryptography. It would be unfair to criticize him for not seeing the future. However, he seems to not see the *synergy* that math has with other fields, even in his own time. Physics was the inspiration for much interesting mathematics. Games of chance were the inspiration for much of Probability.

I view Pure Math, Applied Math, Physics, and Computer Science as all interacting with each other with fuzzy boundaries; each has its interesting and dull parts. As time goes on we may add more fields to that list such as Chemistry, Biology, Philosophy (I am thinking of logic, but there could be other parts), and History (see

`https://blog.computationalcomplexity.org/2013/04/a-nice-case-of-interdisciplinary.html`
for an example).

3 An Applied Mathematician's Apology

Nick Trefethen wrote this book to discuss what applied math is and why it is a worthy endeavor. The book mostly centers on numerical analysis. His main points are (1) pure math underrates applied math, (2) numerical analysis is *interesting*, and (3) some work in numerical analysis is not practical.

Point (1) he makes by looking at the list of Fields Medal winners and noting that not only are none of them in Applied Math [3] , but he has only read the work of one of them (Ahlfors). He also has some anecdotes of pure mathematicians disparaging applied mathematicians. However, my sense is that in recent years this has gotten better. Pure mathematicians are aware of, and respect, for example, the P vs. NP problem. And one of the Millennium prize problems is squarely in applied math, dealing with the Navier-Stokes equations.

One odd point that may support his contention: Trefethen invented and guided the development of a numerical analysis package `Chebfun` from start to finish. This package is widely used and very practical. On his Wikipedia page there is no mention of `Chebfun`.

Regarding point (2), I suspect that for most of my readers the term *Numerical Analysis* evokes the thought *dealing with errors, ugh.* This is a misconception. The author defines the term:

Numerical Analysis is the study of algorithms for problems in continuous mathematics.

The author then gives very good examples of *interesting* results in numerical analysis. We give one here but also relate the authors story of how the study of it went off the rails.

Say you want to interpolate a function f by a polynomial p on the interval $-1 \leq x \leq 1$. Which points should you use? Equally spaced points are terrible. The Chebyshev points, which are clustered around -1 and 1, are excellent. This is interesting and was explained by Carl Runge in 1904. So far, so good.

[3] The following Fields Medal winners are candidates for exceptions: Wendelin Werner (2006, Probability theory and Mathematical Physics), Cédric Villani (2010, PDEs and Mathematial Physics), Martin Hairer (2014, Stochastic partial differential equations), Alessio Figalli (2018, Calculus of Variations and PDEs), Hugo Duminil-Copin (2020, Statistical Physics). Note that the last one got his Fields Medal after the book was written.

But are the Chebyshev points optimal? The answer is no. Okay, then what are the optimal points? Bernstein made a conjecture about this in 1931 that was solved in 1981 by Kilgore and de Boor. Great! So can we now do interpolation much better? No. The improvement is 0%. Hence the work after Runge was academic and has no real-world impact. Yet even if it has no application, is it worth knowing. I leave that as an open question.

The book gives other examples of interesting applied mathematics and sometimes, even when work went astray, where that work led to. Also the book tells the author's own story of how he got into the field and how he thinks about research.

4 Opinion

Hardy's book has some interesting points to make, even if you disagree with them; however, he seems to go on and on at times before getting to his point, which is not worth it in the end. It's dull reading, almost as dull as he claims applied mathematics is. Should you read it? Oddly enough, yes, to see how one could try to defend pure math. Should you buy it? I borrowed mine from a colleague in the math department and I recommend you do the same.

Trefethen's book is interesting. His life story is fascinating, and the math he talks about made me curious to learn more. He makes the point that Numerical analysis (and to a lesser extent applied math) is fundamentally interesting. You should buy and read his book.